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is stimulated; he opposes both theoretical and experimental considerations. His experiments, executed in co-operation with several of his students, consisted in the voluntary performance of preconceived drawing and tracing movements (circles, triangles, squares, etc.), with one or both hands; then in occupying the attention with one hand only and observing the behavior of the other hand; or in withdrawing the attention altogether and observing the behavior of both hands, etc., through the various combinations available. His results lead him to the conclusion that in movements up and down and away from the body, such symmetrical movements do not occur; but that in movements right and left from the body such movements do occur. These latter cases he explains as due to the maintaining of the equilibrium of the body. His conclusion is that "inborn symmetrical co-ordinations of the muscles of the extremities do not exist." [This conclusion is by no means proved by M.'s interesting experiments. In the first place, the present writer finds it almost impossible to keep the attention so constant as not to interfere with the so-called "Mitbewegungen." Further, if Waller's result be true, there would be a certain residual discharge in the muscle after voluntary attention is withdrawn from it and this ought to give a certain amount of movement, symmetrical or asymmetrical, according to the voluntary movement. It is quite possible that there is such an element, but that it is drowned in the grosser tensions due to equilibrium, maintenance of balance, required habit, etc. Such a tendency could only be measured by a graphic record under conditions which ruled out the grosser sources of error; not by the rough explanation of a group of people standing or seated around a table. But, more than this, is experiment on adults likely to throw any light on this question at all? Everybody admits that our adult movements are massed in asymmetrical co-ordinations, which represent the strongest dynamic tendencies. There are, also, facts on the affirmative side of the question, such as "crossed reflexes" (see this Journal V. '92, p. 84). In the case of infants we have "crossed" responses in sleep (see my observations in *Science* XIX., 1892, p. 16, and Preyer's *Mind of the Child*, I., p. 207 ff). M.'s citation (p. 195) of my observations in connection with the development of right-handedness (resumed above) does not take account of the fact that in reporting these observations I added, a little later on, "In many cases the left hand followed slowly upon the lead of the right;" this was also true in the cases in which the left hand led—the right followed after in the same direction. A crucial test of the general question might be reached by Gotch and Horsley's new electrical method; the galvanometer showing to what extent, if at all, the cortical stimulation of a muscular group affects the motor nerves of the opposite side. As far as Gotch and Horsley's results on cats and monkeys bear on the question, they indicate that the tendency to bilateral performances is relative to the intensity of the stimulation—what we would expect from the general principle of diffusion. (Gotch and Horsley, "On the Mammalian Nervous System, etc.," Croonian Lecture, *Phil. Trans.*, 1891.)]

SCHENCK, *Über den Erschlaffungs Process des Muskels* Pflüger's *Archiv*, LII., 1892, p. 117.

S. asks the question: Why is it that the down-slope of the curve of contraction of a muscle exhausted by cooling is as steep as its up-slope, while the down-slope of that of a voluntarily exhausted muscle is not as steep as its up-slopes? He surmises that the less

abrupt down-slope of the voluntarily fatigued muscle is due to the greater consumption of its "reserve elements," apart from the production of lactic acid (I. Rauke). He asks the question, accordingly, whether the relaxation of a muscle in general takes place more slowly when its reserve elements are fewer. This he investigates by an experiment, the conditions of which are to compare the curve of a voluntarily fatigued muscle with that of a muscle whose excitability is reduced by lactic-acid artificially, i. e., in such a way as not to reduce the "reserve-elements" of the preparation. He injected the gastrocnemius of a frog with a .125% lactic acid solution. Its down-slope, like that of the cooled muscle, was less steep than that of the normally fatigued muscle. The same resulted with muscles injected with .1 to .2% soda. The rest of the paper is devoted to a discussion of theories of the molecular processes of muscular contraction and relaxation.

SCHENCK, *Ein apparat zur Verzeichnung von Länge und Spannung des Muskels*, Ibid, p. 108.

The title of this article indicates its contents.

WALLER, *On the Inhibition of Voluntary and Electrically Excited Muscular Contraction by Peripheral Excitation*, Brain, LVII., 1892, 35.

Waller asks whether the diminution in the force of a voluntary muscular contraction brought about by the superposition of direct electrical excitation is due to central inhibition (Fick), or to peripheral inhibition in the body of the muscle (Mosso), or to the excitation of the antagonists. Experimenting on the flexor muscles of the forearm and taking records, both by the dynamograph and "bag recorder" (see description in Brain, 1891, 206), he finds that there is a diminution in the maximum voluntary longitudinal effect, due to the electrical stimulation, but an increase in the maximum voluntary lateral effect. Further, that voluntary contraction superposed upon maximum direct foridization increases both the longitudinal and lateral effects, but that while the sense of maximum direct plus voluntary longitudinal effects is less than the maximum voluntary alone, the sense of the maximum direct plus voluntary lateral effects is more than the maximum of either taken alone. He explains these results largely by the stimulation of the antagonists (extensors); supporting this view by researches on the elevator muscles of the lower jaw, which have no antagonists (in which case the phenomenon in question does not appear), and on the flexors and extensors of the arm (which when both directly stimulated reproduce the phenomenon). A farther question is: Does the cessation of voluntary contraction involve simply a cessation of central voluntary emission, or a stimulation of the antagonists? Waller holds that the former is the main effect. He finds that in cases where the antagonist (extensor) is contracted, there is, on the release of the muscle (flexor), a prolongation of the lateral effect as compared with the longitudinal effect; but in voluntary release of the flexors, there is no such prolongation of the lateral effect. He also finds that in simultaneously grasping with one hand and letting go with the other, the two effects (curves) begin to appear simultaneously; but if the agent in each process be foridization, the muscle stimulated begins to contract before the released muscle begins to cease contracting. Turning from the effects of induced to those of galvanic currents in connection with voluntary contraction, he reaches conclusions which confirm Pflüger's and Waller and De Walleville's earlier results (Phil. Trans., Royal Soc., 1882). The general result is that "active arrest of action, i. e., true physiological inhibition of voluntary muscle, has not yet been demonstrated."